Response for the Review (acp-201456)

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Dear Reviewer,

Thank you for your comments on our paper. We have introduced all minor corrections. Below we present responses to your specific comments.

1. Validity of simulated transport, especially for passive ozone

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More details of the transport simulation setup are presented in section 4.1. The transport is verified by comparison with N2O observations.

The inclusion of the vertical transport does of course improve the model comparison, however, the difference e.g. of 20 ppbv on day 90 at 600K is probably caused probably by too strong mixing into the vortex.

This could be a source of error in the later results when using the passive ozone tracer. Especially at latitudes below 65N where SMILES observes mixing may even stronger as small-scale filaments with high PV may be represented worse by a too diffusive model. (This could be checked e.g. by plotting a figure like fig.6 but with data restricted to geographical latitudes below 65N).

[Since I didn't bring this up clearly in the first review, I do not insist on this point]

>> We agree with the point. However, we think that the main reason for the difference between the assimilated N2O and modeled N2O is rather because of underestimation of the descent inside the vortex. As in fig. 5, the modeled N2O shows slightly higher (~5ppbv and ~10ppbv until/after 40 DOY) than the assimilated N2O and the difference seems to slowly increase in time. We used the SLIMCAT heating rate to derive a descent rate. The provided heating rate, however, was produced twice per day. We interpolated linearly between two values in time that is a reasonable approximation during polar night and the stable vortex. It may, however, cause an underestimation of

the descent under other circumstances.

In addition, we believe that our model is good enough even below 65N because the difference between model and assimilation on day 90 is still smaller than the standard deviation inside the vortex. We checked the suggested plot and we drew the same conclusion.

The derived ozone loss is calculated by subtracting passively transported ozone from assimilated ozone. The value of passive ozone very critically depends on the initialization and the transport characteristics of the model. Especially when only air masses south of 65 degrees are used, that may contain high-PV vortex filaments in which the transport and mixing characteristics of the model is challenged.

line 188: Does that mean that you initialized the passive ozone with ECMWF ozone values? I thought it was initialized from assimilated ozone at 1 December 2009. Please clarify.

>> The passive ozone was initialized from assimilated ozone at 1 December 2009. As in line 174, we had the one-month prior calculation for active and passive fields. The "initial ozone field" in line 188 means the ozone field on 1 November 2009. We have added the specific date in the new manuscript.

Figure 5 differs slightly from the previous fig 4. Is it just some other smoothing/ plotting routine or a different simulation? Please correct also the panel titles of fig 5.

>> The differences between fig.5 and previous fig.4 just come from different smoothing settings. The data used for the plotting are the same.

2. Definition of modified PV

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line 194 modified PV

There are different definitions of modified PV in the literature (Lait et al.; Mueller and Guenther, 2003; Manney et al.)

Please specify method and reference level.

>> We used Lait's PV in the study. We have added the reference in the new

manuscript.

Changes in the results between averaging over the PV criterion rather than EQL>70N suggests that the difference between EQL and PV criterion does change significantly with altitude. Is this PV criterion meaningful for the whole altitude range and how does it differ with respect to derived vortex size?
>>Yes, the PV criterion is useful for all altitudes. As you can see in figure 4,7 and 8, the vortex size of PV criterion at 600K was almost same or slightly larger than EQL criterion until the middle of February (45 DOY) and became smaller after the day. However, we do not see any altitude difference.

At all locations in the paper where PV is mentioned, it should be made clear, that modified PV is referred to.
>> Fixed

3. Statements to PSC types

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287ff: Even though it is not central to this paper, it has been shown that the observed denitrification can indeed be explained without ice formation on NAT (Grooss et al., 2014). There are more publications that focus on NAT and ice formation (Engel et al., Hoyle et al.).
However, the "type of PSCs" is not really important in this context, as already cold binary aerosols can activate chlorine (e.g. Drdla and Müller, 2012).
Thus, that I would suggest leaving out this sentence (also line 36/37).
>> We still keep line 36/37, but removed the line 287.

4. Nighttime ClO

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293ff: It is good, that the authors have incorporated this argument.
However, I would see the elevated nighttime ClO only as a sign of chlorine activation. A quantitative evaluation would require modelling, but at least

information of zenith angle and temperature of each observation. I don't think this should be done here. But without that, the number of globally averaged ClO is not very meaningful and should probaly be left out.
>>Sorry but we do not fully understand the comment. We have decided to keep the sentences. We agree that the main reason to mention the ClO is to emphasize that the activation has occurred.

5. Interpretation

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l. 300 However...
This is formulated as if this would be a surprising new result to have ozone loss by other mechanisms that chlorine chemistry. In the later cited references refer to the NOx catalyzed ozone loss. I suggest change the wording to something like "The upper level (..) ozone loss is not due to chlorine chemistry..."
>> We have changed the wording.

Also line 158ff could be read as if it would be a new finding that there is descent in the polar vortex. Formulate rather as "The descent of air in the polar vortex caused by radiative cooling during polar night had not be taken into account in the previous model version. It is, however, necessary for a correct evaluation of ozone loss..."
>> We have changed the wording.

Minor issues

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line 167: change omega to w
line 212: shown are N2O volume mixing ratios, not concentrations
line 227: capitalize Ozone
line 269: The second ozone loss period took...

>> All minor issues are corrected in the new manuscript.